

EXECUTIVE SUMMARY

Job Corps stands out as the nation's largest, most comprehensive education and job training program for disadvantaged youths. It serves disadvantaged youths between the ages of 16 and 24, primarily in a residential setting. It provides comprehensive services—basic education, vocational skills training, health care and education, counseling, and residential support. Each year, Job Corps serves more than 60,000 new participants in about 120 centers nationwide, at a cost of about \$1.5 billion.

The National Job Corps Study has been conducted since 1993 under contract with the U.S. Department of Labor (DOL). It is intended to provide Congress and program managers with the information they need to assess how well Job Corps attains its goal of helping students become more responsible, employable, and productive citizens.

The cornerstone of the National Job Corps Study was the random assignment of all youths found eligible for Job Corps to either a program group (who could enroll in Job Corps) or a control group (who could not). The research sample consists of approximately 9,400 program group members and 6,000 control group members randomly selected from among nearly 81,000 applicants nationwide. Random assignment took place between late 1994 and early 1996. The survey data for the evaluation come from interviews conducted at baseline (shortly after random assignment), and at 12, 30, and 48 months after random assignment. The response rate to the 48-month interview was about 80 percent (81 percent for the program group and 78 percent for the control group). Program impacts were estimated by comparing the mean outcomes of program and control group members.

The survey data indicate that Job Corps generated positive impacts on earnings—the key outcome for the study—beginning in the third year after random assignment, and the impacts persisted without decline through the end of the four-year follow-up period. Beneficial program impacts were found broadly across youth subgroups. A benefit-cost analysis based on impact estimates from the survey data found that the benefits to society from the program exceed its costs. However, this finding requires a key assumption—that the earnings gains observed during the last year of the observation period will persist with little decay.

This report presents findings from an analysis of *administrative* earnings records. These data allow us to address two questions:

1. *Do survey and administrative earnings data yield similar impact estimates on employment and earnings during the periods covered by both data sources?*
2. *What are estimated impacts on earnings and employment in the two and a half years beyond the four-year period covered by the survey?*

Two sources of administrative data were collected for the study: (1) annual social security earnings (SER) data reported by employers to the Internal Revenue Service (IRS) and Social Security Administration (SSA), and (2) quarterly wage records reported by employers to state

unemployment insurance (UI) agencies in 22 randomly selected states. The SER data cover calendar years 1993 to 2001. The years 1995 to 1998 pertain roughly to the four-year period covered by the survey, and the years 1999 to 2001 pertain to the post-survey period (that is, years 5, 6, and 7 after random assignment). The UI data cover the 1999 to 2001 period only. The SER and UI data cover nearly all workers in formal jobs. Earnings from informal jobs are not covered.

IMPACT FINDINGS DURING THE PERIOD COVERED BY THE SURVEY

The pattern of the estimated impacts using the survey and administrative data are similar in periods covered by both data sources. According to both the survey and SER data, the estimated earnings impacts are negative in 1995 and 1996 (when the program group was enrolled in Job Corps) and positive and statistically significant in 1997 and 1998 (Table 1).

However, the survey-based impact estimates are larger and more often statistically significant. Reported earnings *levels* are much higher according to the survey data for a large percentage of sample members (Tables 1 and 2). We find larger differences between the earnings levels reported in the survey and administrative data than were found in previous studies using similar populations. One possible explanation for this finding is that the National Job Corps Study was conducted during a period of strong economic growth, which may have increased the earnings sample members received from informal jobs.

Annual employment rates are similar using the survey and administrative data, but quarterly employment rates are much higher using the survey data. The annual employment rate in 1998 is about 80 percent according to both the survey and SER data (Table 1). However, the quarterly employment rates in quarters 15 and 16 after random assignment are substantially higher using the survey than UI data (Table 2).

Differences in the 1998 earnings gains using the survey and SER data are due in roughly equal parts to reporting differences between the two data sources and to nonresponse bias. The estimated 1998 earnings gain is 10.4 percent according to the survey data and 3.9 percent according to the SER data (Table 1). Using the *sample of respondents to the 48-month interview only*, the SER-based earnings gain increases from 3.9 to 6.9 percent (Table 3), which is still smaller than the 10.4 percent survey-based figure. Thus, the residual is due to reporting differences between the two data sources that are slightly *greater* for the program than control group. We estimate that about 46 percent of the difference between the 1998 earnings gains using the survey and SER data is due to interview nonresponse bias, and 54 percent is due to reporting differences between the two data sources.

EXAMINING REPORTING DIFFERENCES BETWEEN THE SURVEY AND ADMINISTRATIVE DATA

We have seen that the pattern of impact findings using the administrative and survey data is similar in periods covered by both data sources. However, the estimated impacts are larger using the survey data. This is due primarily to reported earnings *levels* that are substantially higher according to the survey than administrative data for most sample members.

TABLE 1
 IMPACTS ON CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES
 FOR THE FULL SAMPLE, BY DATA SOURCE

| Outcome Measure | Data Source | | | | | | | | | | | | | |
|--|---------------|---------------|-------------------------------|-------------------|---|-------------------------------|---------------|---------------|--|---------------|-------------------------------|---------------|---------------|-------------------------------|
| | Survey Data | | | | Annual Social Security Earnings Records | | | | Quarterly UI Earnings Records from 22 States | | | | | |
| | Program Group | Control Group | Estimated Impact ^a | Program Group | Control Group | Estimated Impact ^a | Program Group | Control Group | Program Group | Control Group | Estimated Impact ^a | Program Group | Control Group | Estimated Impact ^a |
| Average Calendar Year Earnings (in 1995 Dollars) | | | | | | | | | | | | | | |
| 1993 | | | | 1,009.6 | 1,013.9 | -4.3 | | | | | | | | |
| 1994 | | | | 1,590.3 | 1,542.4 | 47.9 | | | | | | | | |
| 1995 | | | | 1,758.4 | 2,026.5 | -268.1*** | | | | | | | | |
| 1996 | 5,144.8 | 5,728.8 | -584.0*** | 3,096.7 | 3,273.5 | -176.8*** | | | | | | | | |
| 1997 | 8,110.5 | 7,818.6 | 291.9* | 4,540.1 | 4,368.4 | 171.8** | | | | | | | | |
| 1998 | 10,295.6 | 9,324.1 | 971.6*** | 5,803.9 | 5,584.1 | 219.8** | | | | | | | | |
| 1999 | | | | 6,652.9 | 6,619.9 | 32.9 | | | | | | | | |
| 2000 | | | | 7,526.7 | 7,544.1 | -17.4 | | | | | | | | |
| 2001 | | | | 7,678.7 | 7,671.7 | 7.0 | | | | | | | | |
| Percentage Employed in Calendar Year | | | | | | | | | | | | | | |
| 1993 | | | | 42.9 | 43.0 | -0.1 | | | | | | | | |
| 1994 | | | | 59.5 | 58.8 | 0.7 | | | | | | | | |
| 1995 | | | | 89.2 ^b | 73.3 | 15.9*** | | | | | | | | |
| 1996 | 70.4 | 74.5 | -4.2*** | 88.7 ^b | 78.3 | 10.4*** | | | | | | | | |
| 1997 | 77.7 | 76.9 | 0.8 | 83.5 | 81.4 | 2.1*** | | | | | | | | |
| 1998 | 81.4 | 78.9 | 2.4*** | 84.5 | 83.2 | 1.3** | | | | | | | | |
| 1999 | | | | 84.4 | 83.0 | 1.4** | | | | | | | | |
| 2000 | | | | 83.5 | 82.8 | 0.6 | | | | | | | | |
| 2001 | | | | 80.0 | 79.7 | 0.3 | | | | | | | | |
| Sample Size | 6,828 | 4,485 | 11,313 | 9,264 | 5,874 | 15,138 | 4,613 | 2,855 | 7,468 | 4,613 | 2,855 | 7,468 | 4,613 | 2,855 |

TABLE 1 (continued)

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security earnings records for the full research sample; and (3) quarterly UI earnings records from the following 22 randomly selected states for those who signed the records release consent form: AR, AZ, CA, FL, ID, IL, KS, LA, MD, ME, MI, MO, MS, NC, NE, NJ, OH, OK, SC, TX, VA, and WA.

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
 2. All estimates were calculated using sample weights to account for (1) the sample design (for all three data sources), (2) the survey design and interview nonresponse (for the survey data), and (3) the selection of states to the UI sample and nonresponse to the records release form (for the UI data). Standard errors of the estimates account for design effects due to the unequal weighting of the data and clustering of areas for in-person interviews at baseline (for the survey data) and the selection of states (for the UI data).

^aThese estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members. The UI-based impact estimates are measured as the weighted sum of the outcome measure for the full sample divided by the number of youths who lived in the 22 selected states at application to Job Corps.

^bEmployment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

***Significantly different from zero at the .01 level, two-tailed test.

TABLE 2
 IMPACTS ON EARNINGS AND EMPLOYMENT USING SURVEY AND UI DATA
 FOR THE FULL SAMPLE, BY QUARTER AFTER RANDOM ASSIGNMENT

| Outcome Measure | Data Source | | | | | |
|---|------------------|------------------|----------------------------------|---|------------------|----------------------------------|
| | Survey Data | | | Quarterly UI Earnings Records from 22 States | | |
| | Program Group | Control Group | Estimated Impact ^a | Program Group | Control Group | Estimated Impact ^a |
| Average Earnings, by Quarter After Random Assignment (in 1995 Dollars) | | | | | | |
| 1 | 565.4 | 851.4 | -286.0*** | | | |
| 4 | 1,201.0 | 1,378.4 | -177.4*** | | | |
| 8 | 1,992.8 | 1,909.8 | 83.0* | | | |
| 12 | 2,550.7 | 2,321.5 | 229.2*** | | | |
| 13 | 2,669.4 | 2,444.1 | 225.4*** | | | |
| 14 | 2,727.6 | 2,524.2 | 203.5*** | | | |
| 15 | 2,778.3 | 2,564.2 | 214.1*** | 1,396.5 | 1,299.1 | 97.3 |
| 16 | 2,827.0 | 2,591.6 | 235.4*** | 1,414.8 | 1,382.0 | 32.8 |
| 17 | | | | 1,449.7 | 1,470.9 | -21.2 |
| 18 | | | | 1,508.9 | 1,511.6 | -2.7 |
| 19 | | | | 1,545.6 | 1,553.5 | -7.9 |
| 20 | | | | 1,568.6 | 1,593.0 | -24.4 |
| 21 | | | | 1,632.6 | 1,677.3 | -44.8 |
| 22 | | | | 1,707.8 | 1,772.0 | -64.3 |
| 23 | | | | 1,721.7 | 1,775.5 | -53.8 |
| 24 | | | | 1,800.4 | 1,857.7 | -57.3 |
| 25 | | | | 1,856.2 | 1,909.0 | -52.8 |
| 26 | | | | 1,909.0 | 1,955.6 | -46.6 |
| Percentage Employed, by Quarter After Random Assignment | | | | | | |
| 1 | 33.2 | 42.1 | -8.9*** | | | |
| 4 | 49.8 | 57.7 | -7.9*** | | | |
| 8 | 59.0 | 57.9 | 1.2 | | | |
| 12 | 66.2 | 63.0 | 3.2*** | | | |
| 13 | 66.8 | 63.4 | 3.4*** | | | |
| 14 | 67.5 | 65.1 | 2.4*** | | | |
| 15 | 69.2 | 65.6 | 3.6*** | 55.0 | 52.6 | 2.3 |
| 16 | 71.1 | 68.7 | 2.4*** | 55.4 | 55.1 | 0.3 |
| 17 | | | | 55.2 | 54.9 | 0.3 |
| 18 | | | | 54.9 | 55.9 | -1.0 |
| 19 | | | | 55.2 | 56.3 | -1.1 |
| 20 | | | | 53.6 | 55.5 | -1.8 |
| 21 | | | | 53.9 | 55.5 | -1.6 |
| 22 | | | | 55.3 | 57.1 | -1.7 |
| 23 | | | | 55.5 | 57.4 | -2.0 |
| 24 | | | | 55.3 | 58.5 | -3.3** |
| 25 | | | | 56.5 | 58.6 | -2.1 |
| 26 | | | | 58.1 | 61.0 | -2.8 |
| Sample Size | 6,828 | 4,485 | 11,313 | 4,613 | 2,855 | 7,468 |

TABLE 2 (continued)

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; and (2) quarterly UI earnings records from the following 22 randomly selected states for those who signed the records release consent form: AR, AZ, CA, FL, ID, IL, KS, LA, MD, ME, MI, MO, MS, NC, NE, NJ, OH, OK, SC, TX, VA, and WA.

Notes: 1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.

2. All estimates were calculated using sample weights to account for (1) the sample design (for both data sources), (2) the survey design and interview nonresponse (for the survey data) and (3) the selection of states to the UI sample and nonresponse to the records release form (for the UI data). Standard errors of the estimates account for design effects due to the unequal weighting of the data and clustering of areas for in-person interviews at baseline (for the survey data) and the selection of states (for the UI data).

^aThese estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

***Significantly different from zero at the .01 level, two-tailed test.

TABLE 3

IMPACTS ON CALENDAR YEAR EARNINGS AND EMPLOYMENT RATES FOR RESPONDENTS
TO THE 48-MONTH INTERVIEW, ACCORDING TO THE SURVEY AND SER DATA

| Outcome Measure | Data Source | | | | | | | | | | | |
|--|--------------------------------|-------------------------------|--------------------------------|-------------------|--------------------------------|---|--------------------------------|-------------------------------|--------------------------------|---------------|-------------------------------|-----------|
| | Survey Data | | | | | Annual Social Security Earnings Records | | | | | | |
| | 48-Month Interview Respondents | | 48-Month Interview Respondents | | 48-Month Interview Respondents | | 48-Month Interview Respondents | | Respondents and Nonrespondents | | | |
| Program Group | Control Group | Estimated Impact ^a | Program Group | Control Group | Estimated Impact ^a | Program Group | Control Group | Estimated Impact ^a | Program Group | Control Group | Estimated Impact ^a | |
| Average Calendar Year Earnings (in 1995 Dollars) | | | | | | | | | | | | |
| 1993 | | | | 1,001.9 | 1,028.8 | -27.0 | 1,009.6 | 1,013.9 | | | | -4.3 |
| 1994 | | | | 1,587.0 | 1,550.6 | 36.4 | 1,590.3 | 1,542.4 | | | | 47.9 |
| 1995 | | | | 1,772.1 | 2,033.6 | -261.5*** | 1,758.4 | 2,026.5 | | | | -268.1*** |
| 1996 | 5,144.8 | 5,728.8 | -584.0*** | 3,175.1 | 3,330.9 | -155.9** | 3,096.7 | 3,273.5 | | | | -176.8*** |
| 1997 | 8,110.5 | 7,818.6 | 291.9* | 4,696.4 | 4,443.2 | 253.2** | 4,540.1 | 4,368.4 | | | | 171.8** |
| 1998 | 10,295.6 | 9,324.1 | 971.6*** | 6,045.8 | 5,655.3 | 390.5*** | 5,803.9 | 5,584.1 | | | | 219.8** |
| 1999 | | | | 6,925.7 | 6,754.0 | 171.7 | 6,652.9 | 6,619.9 | | | | 32.9 |
| 2000 | | | | 7,835.0 | 7,644.8 | 190.2 | 7,526.7 | 7,544.1 | | | | -17.4 |
| 2001 | | | | 8,017.6 | 7,863.7 | 154.0 | 7,678.7 | 7,671.7 | | | | 7.0 |
| Percentage Employed in Calendar Year | | | | | | | | | | | | |
| 1993 | | | | 42.6 | 42.6 | 0.0 | 42.9 | 43.0 | | | | -0.1 |
| 1994 | | | | 60.0 | 58.7 | 1.3 | 59.5 | 58.8 | | | | 0.7 |
| 1995 | | | | 90.0 ^b | 73.0 | 17.0*** | 89.2 ^b | 73.3 | | | | 15.9*** |
| 1996 | 70.4 | 74.5 | -4.2*** | 89.6 ^b | 78.7 | 10.9*** | 88.7 ^b | 78.3 | | | | 10.4*** |
| 1997 | 77.7 | 76.9 | 0.8 | 85.1 | 82.7 | 2.4*** | 83.5 | 81.4 | | | | 2.1*** |
| 1998 | 81.4 | 78.9 | 2.4*** | 85.9 | 84.4 | 1.5** | 84.5 | 83.2 | | | | 1.3** |
| 1999 | | | | 86.0 | 84.6 | 1.4** | 84.4 | 83.0 | | | | 1.4** |
| 2000 | | | | 85.2 | 85.0 | 0.2 | 83.5 | 82.8 | | | | 0.6 |
| 2001 | | | | 81.4 | 81.7 | -0.3 | 80.0 | 79.7 | | | | 0.3 |
| Sample Size | 6,828 | 4,485 | 11,313 | 6,772 | 4,451 | 11,223 | 9,264 | 5,874 | 15,138 | 15,138 | | |

TABLE 3 (continued)

Sources: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security earnings records for the full research sample.

- Notes:
1. Blank entries signify that figures are not applicable because data were not available or sample sizes were too small to generate precise estimates.
 2. All estimates were calculated using sample weights to account for (1) the sample design, (2) the survey design and interview nonresponse (for the survey sample). Standard errors of the estimates account for design effects due to the unequal weighting of the data and clustering of areas for in-person interviews at baseline.

^aThese estimated impacts pertain to eligible applicants, and are measured as the difference between the weighted means for program and control group members.

^bEmployment rates are high for the program group in 1995 and 1996 because student pay that Job Corps students receive while enrolled in the program is reported to the government.

*Significantly different from zero at the .10 level, two-tailed test.

**Significantly different from zero at the .05 level, two-tailed test.

***Significantly different from zero at the .01 level, two-tailed test.

There are several possible explanations for the higher reported earnings levels in the survey data. First, informal and some formal jobs are not covered by the administrative records data but may be captured in the survey data. Second, some survey respondents may have over-reported their earnings and employment levels due to recall error or other reasons. Third, some employers may have inaccurately reported (or not reported) sample members' earnings to the government. Finally, the administrative records data may have missed earnings from sample members with SSNs (or other identifying information) that were incorrectly reported by employers or sample members.

To examine these explanations, we compared individual employment and earnings measures based on the UI and survey data in quarter 16 after random assignment (the most recent overlapping period). We did not use the SER for these analyses, because SSA does not release earnings records for individuals, but only for groups of individuals.

The analysis focused on the following questions: (1) Why are quarter 16 employment levels 13 percentage points higher and the number of jobs per worker 20 percent higher in the survey data than in the UI data? and (2) Why are quarter 16 earnings levels nearly 40 percent higher in the survey data than in the UI data, even for those with the same number of reported jobs according to both data sources?

Errors in sample members' Social Security Numbers (SSNs) partly account for the higher employment levels in the survey than the UI data. Unlike SSA, UI agencies do not verify reported SSNs before matching to their earnings records. This problem is exacerbated by the fact that about 12 percent of our sample members reported multiple SSNs over the course of the study. Thus, the UI wage records could miss earnings from persons with SSNs that were incorrectly reported by employers or sample members. Our finding that employment rates and mean earnings are somewhat lower in the UI than the SER data support this explanation.

The non-coverage of some formal jobs under the UI program appears to account for only a small portion of the gap between the employment rate as measured by the survey and UI data. The UI data do not cover workers in all formal jobs (for example, federal workers, military staff, self-employed persons, some agricultural labor, and domestic service workers). Using workers in the survey data, we find that those who were likely to be in non-covered formal jobs are somewhat less likely to have a record in the UI data than those who were likely to be in covered formal jobs. However, many of those likely to be in covered jobs do *not* have a record in the UI data. Thus, differences in survey and UI match rates across occupations are smaller than expected. These findings could be due in part to errors in classifying jobs reported in the survey into occupational categories, a result of limited survey information on the nature and title of jobs held by sample members.

The survey data provides only weak evidence that the higher employment rate is due to informal jobs. Sample members with informal (casual or cash-only) jobs were asked to report them in the survey, but these jobs were not likely to have been reported in the UI data. To examine the extent to which informal jobs explain the higher survey-based employment rate, we compared the characteristics of jobs reported in both the survey and UI data with the characteristics of jobs reported in the survey data only. As expected, average hourly wages and the likelihood of having available fringe benefits on the job were slightly lower for the survey-

only group. However, job tenure and usual hours worked were similar for the two groups of workers. Consequently, the differences in the characteristics of jobs held by the two groups of workers were smaller than expected.

Substantial unobserved factors account for the employment rate differences according to the survey and UI data. Few explanatory variables have predictive power in a multivariate regression model of whether survey-based jobs are reported in the UI data. Age, fertility status, marital status, health status, education level, welfare receipt status, and crime and drug use experiences do not significantly affect whether survey-reported jobs are reported in the UI data. Furthermore, only a few of the employment-related variables are statistically significant.

The likely over-reporting of hours worked in the survey data plays an important role in explaining the higher *earnings per job* levels in the survey than UI data. The level of earnings over a given period is the product of (1) the number of weeks worked on the job during the period, (2) the usual hours per week worked, and (3) the hourly wage rate. An examination of the association between each of these earnings components (as measured by the survey) and the *ratio* of average survey-to-UI earnings found that the survey-to-UI ratios increase with the number of hours worked as reported in the survey, but not with hourly wage rates or weeks worked. Moreover, the average worker reported working about 42 hours per week on their most recent job in quarter 16, and more than three-quarters reported working at least 40 hours—figures that are *higher* than the corresponding figures for all U.S. workers.

Some evidence suggests that earnings differences between the survey and UI data are smaller for those in stable jobs than less stable ones. We found some support for the hypothesis that earnings differences using the survey and UI data are smaller for sample members who held stable jobs. Earnings differences are much smaller for those with longer job tenure. Furthermore, the differences are somewhat larger for those in occupations that are more likely to have irregular hours (such as construction and private household occupations).

Few differences in findings occur between the program and control groups. Reporting differences between the survey and UI data are slightly larger for the program than control group, resulting in percentage earnings gains that are slightly larger according to the survey than UI data. However, no evidence was found that the program group was more likely than the control group to hold informal jobs or formal jobs not covered by the UI program; the distribution of the occupations of the jobs held by program and control group members in quarter 16 is very similar. Furthermore, there is no evidence that the program group was more likely than the control group to over-report hours worked on their jobs.

INTERVIEW NONRESPONSE BIAS

As discussed, we found using the SER data that post-program earnings impacts for 48-month interview respondents are larger than for interview nonrespondents. These results suggest that the survey-based earnings impact estimates are biased upwards. What accounts for the interview nonresponse bias? The two possible explanations are:

1. ***Differences in the baseline characteristics of respondents in the program and control groups that are correlated with earnings.*** If interview respondents in the program group were drawn from a somewhat more advantaged subpopulation of the full program group than was the case for interview respondents in the control group, the survey-based impact estimates would be biased upwards.
2. ***True differences in the earnings impacts for survey respondents and survey nonrespondents.*** If earnings impacts are truly larger for survey respondents than survey nonrespondents, the survey-based earnings impacts would be biased upwards even if the observable and unobservable characteristics of respondents in the program and control groups are similar.

While it is difficult to disentangle these two possible explanations, the data support more strongly the explanation that the bias is caused by true differences in the earnings impacts for survey respondents and nonrespondents. Several pieces of evidence indicate that respondents in the program and control groups are comparable, suggesting that the former explanation cannot fully account for the nonresponse bias. First, the 48-month interview response rates are similar for the program and control groups. Second, the distributions of a large number of observable baseline characteristics and of the number of months until the 48-month interview was completed are similar for respondents in the program and control groups. Third, impact estimates based on the survey data are similar for subsamples of interview respondents that were formed to *equalize* the interview response rate for the program and control groups by selecting those who completed interviews *first*. Finally, impact estimates based on the SER data are similar using 12-month and 48-month interview respondents, even though the response rate was much higher to the 12-month interview.

The available evidence suggests also that earnings impacts truly differ for interview respondents and nonrespondents, supporting the second explanation for nonresponse bias. Observable baseline characteristics differ somewhat for respondents and nonrespondents, and mean earnings levels using the SER data were larger for respondents than nonrespondents during the post-program period. Most importantly, respondents had somewhat higher Job Corps participation levels than nonrespondents and stayed in the program for nearly one month longer on average than nonrespondents.

IMPACT FINDINGS AFTER THE PERIOD COVERED BY THE SURVEY

Based on the administrative data, we find no impacts of Job Corps for the full sample on employment or earnings after the four-year period covered by the survey. The estimated impacts on calendar year earnings in 1999 to 2001 are all near zero and none are statistically significant (Tables 1 and 2). The earnings impacts in the post-survey period for 48-month interview respondents only are also not statistically significant (Table 3).

However, the SER-based earnings impacts for 20- to 24-year-olds at program application appear to have persisted. We find no beneficial SER-based earnings impacts in 2000 and 2001 that are statistically significant for any subgroup. However, positive earnings gains for those 20 to 24 and those with a high school credential at program application persisted with little decay.

BENEFIT-COST ANALYSIS

As Job Corps is an intensive program that aims to make long-term impacts on the lives of the youth it serves, it is important to consider the benefits that may occur after the four-year survey observation period. In our initial benefit-cost analysis based only on survey data, we found that benefits exceed costs by \$17,000 per participant (Table 4). A key assumption underlying this finding was that the impacts on earnings in the observation period would persist without decay for the rest of the average participant's working lifetime. The impact findings using the administrative data, however, place the validity of this assumption in question.

TABLE 4
INITIAL AND REVISED ESTIMATES OF BENEFITS AND COSTS OF JOB CORPS
(1995 Dollars)

| | Initial Estimates: Used Survey Data and Earnings Impacts Assumed Not to Decay | Revised Estimates: Used Adjusted Survey Data and Earnings Impacts ^a Assumed to Decay at Rate Observed in SER Data ^b | |
|-----------------------|--|--|-----------------|
| | Full Sample | Full Sample | 20-24 Year Olds |
| Total Benefits | 30,957 | 3,695 | 14,696 |
| Increased Output | 27,531 | 269 | 17,547 |
| Years 1-4 | 753 | -60 | 588 |
| After Year 4 | 26,778 | 329 | 16,959 |
| Other Benefits | 3,426 | 3,426 | -2,850 |
| Program Costs | -14,128 | -13,844 | -15,193 |
| Net Benefits | 16,829 | -10,150 | -496 |

Source: (1) Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews; (2) annual social security earnings records; and (3) McConnell and Glazerman (2001).

^aEarnings reported on the surveys are adjusted for survey nonresponse and overreporting of hours by 10 percent. The length of time youth are in Job Corps is also adjusted for nonresponse; this affects estimates of program costs and the output produced during vocational training in Job Corps.

^bThe rate of decay in the SER earnings impacts from the fourth year after random assignment to the seventh year after random assignment is 68.3 percent for the full sample and 5.9 percent for the 20 to 24 year olds.

The revised benefit-cost estimates suggest that the benefits to society of Job Corps are smaller than the substantial program costs. The revised estimates are based on the estimated *survey* earnings impacts that are adjusted downward to account for nonresponse bias and the likely overreporting of hours. We assume also that the earnings impacts decay at the same rate after the observation period as the impacts based on the SER data—68.3 percent per year. Under these assumptions, costs exceed benefits by \$10,200 per participant (Table 4). This change in

findings is due to the replacement of the assumption that earnings impacts persist with an assumption, more consistent with the administrative data, that they will decay rapidly. The finding that costs exceeds benefits for the full sample holds under a wide range of reasonable assumptions.

Job Corps may be cost-effective for the older youth whose earnings impacts persisted during the post-survey period. We find that benefits to society are only \$500 lower than program costs for the youth who were 20- to 24-years old at program application, under the assumption that the positive earnings impacts in 1998 to 2001 will decay at the same rate as they do in the SER data for this subgroup (Table 4). While this is our best estimate of the benefits and costs, the finding that costs exceed benefits is sensitive to small changes in assumptions. For example, if we treat the positive impact on arrests for murder for this subgroup as an anomaly, benefits would exceed costs.

Job Corps is still worthwhile for its participants. Job Corps is a good deal for program participants because the value of pay, food, and clothing they receive in the program offsets the earnings forgone while they are enrolled in Job Corps. Thus, the program has important distributional effects.

CONCLUSIONS

Intensive, costly programs like Job Corps can only be expected to show benefits that exceed costs over a relatively long time horizon. Unfortunately, the foundation of empirical evidence to make long-term extrapolations of the profile of earnings in response to programs such as Job Corps simply does not exist. Nonetheless, we are forced to make extrapolation assumptions about future earnings. When we conducted the initial benefit-cost analysis, we assumed that the earnings impact in the last year of the observation period would persist with little decay, and found that program benefits to society exceed program costs. The analysis of the administrative data, however, casts doubt on the validity of this assumption. If true earnings impacts decay at the same rate as observed in the administrative data, our initial conclusion is reversed—the costs of Job Corps exceed its benefits for the full sample, although the program may be cost-effective for the older youth. Job Corps is too costly a program for short-term benefits to exceed costs. However, we still have observed earnings for only five years after the program group left Job Corps. Only further long-term follow up would eliminate all uncertainties about the effectiveness of the program.

The impact findings for the older youth can help guide future program improvement. Job Corps appears to have a longer-term beneficial effect on the earnings of older students than younger ones. Older students remain in Job Corps longer than younger ones, receive more hours of vocational training while enrolled, and are more highly motivated and well-behaved (as reported by program staff). Furthermore, many of the youngest sample members in the control group returned to high school after being rejected from Job Corps, whereas fewer older control group members enrolled in alternative education and training programs. These findings suggest that to improve overall program effectiveness, Job Corps needs to fully address differences by age in program structure and experience, and perhaps, to reassess the target population served by the program.

Finally, it is important to emphasize that the findings presented in this report pertain to the Job Corps program as it operated in 1995 and 1996 (when our program group members were enrolled in Job Corps), and not necessarily to the program as it operates today. There have been a number of significant changes that Job Corps has recently implemented in response to WIA provisions and other factors. For example, more Job Corps centers are now accredited to award high school diplomas, and Job Corps is more focused on providing longer-term support and placement services for their former students. These changes may have improved program effectiveness.